

Unités électriques absolues. By Prof. G. Lippmann. Pp. ii + 240. (Paris: Carré and Naud, 1899.)

THIS treatise is the reproduction of professorial lectures delivered at the Sorbonne in the session of 1884-85, and consists mainly of three parts. The first part deals with the electrostatic system of units, the second with the electromagnetic system, and the third with the electromagnetic theory of light. These are preceded by an introduction, which treats of units in general and the c.g.s. system. At the close of the book are two supplements, dealing respectively with the conservation of electricity and Lippmann's electro-dynamometer.

The treatment is chiefly mathematical, the experimental methods referred to being described in outline. The analysis is, however, simple and the text illustrated by a hundred excellent figures. Indeed, the book is on the whole so good and clear that one regrets the more that the dimensional formulæ have not been brought up to date by embodiment of the progress made in the fourteen years which have elapsed between the delivery of these lectures and their publication. In our view, the value of the book would have been much enhanced by the introduction in it of Prof. Rücker's work on the usually *suppressed* dimensions of μ and k (see paper read before the Physical Society of London, November 24, 1888; NATURE, vol. xxxix. p. 165).

This, impossible in the lectures themselves delivered in 1884-85, was both possible and highly desirable in the book of 1899. E. H. B.

Elementary Practical Physiography (Section II). By J. Thornton, M.A. Pp. viii + 208. (London: Longmans, Green and Co., 1900.)

THIS is an effort to meet the requirements of candidates for the Queen's Scholarship in Section II. of the syllabus of elementary science. Its scope is best described by the sub-title "A Course of Lessons and Experiments in Elementary Science," but it is necessary to add that the only branches of science touched upon are chemistry and astronomy. In both these subjects some knowledge gained by experiment and observation is now expected; but though the author claims to have kept this in view throughout, there is little in the book to entitle it to be called practical. It is true that reference is made to seventy-four experiments in chemistry, but they are for the most part better adapted as suggestions for the teacher than for performance by the student. In the astronomical section an excellent course of reading lessons is provided, but the author has by no means taken sufficient advantage of the opportunity of directing the student's attention to the heavenly bodies themselves. Instead of the descriptions of simple apparatus for making observations which might have been expected, such, for instance, as the measurement of altitude and azimuth, half-a-dozen class-room demonstrations are alone given.

Objection may be taken to the author's statement that "most of the diagrams are new and original"; many of them seem familiar, though they may have been re-drawn for their present purpose. A. F.

Atlas de Photomicrographie des Plantes Médicinales. Par MM. les Drs. Braemer et Suis. Pp. vi + 230; 76 plates. (Paris: Vigot Frères, 1900.)

THIS book consists of series of plates derived from micro-photographs of the ordinary medicinal plants. To the plates relevant to each plant a descriptive text is added, dealing with the morphology of the respective plant. The microscopic sections are very clear and well reproduced.

The book ought to be useful to those interested in *materia medica*; but although we know of no similar work, we are afraid it will only appeal to a relatively small circle of readers.

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LETTERS TO THE EDITOR.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. No notice is taken of anonymous communications.]

Effects of Lightning upon Electric Lamps.

IN a communication to NATURE (p. 391), Prof. Wood pointed out the similarity of the features exhibited in Mr. Webb's photographs to the trails of luminosities exhibited in a picture taken with a moving camera.

To this I replied (p. 413), saying I had understood that the camera was fixed, and calling attention to two features which seemed to show that the phenomenon was real.

An independent suggestion similar to that of Prof. Wood, from another quarter, accompanied by photographs purposely taken with a moving camera,¹ subsequently came before me. This helped to arouse suspicion, and it occurred to me as conceivable that though the camera was fixed, Mr. Webb might as a matter of convenience have taken it up before he capped it; and if so, trails might have been left from the short exposure between lifting and capping. I wrote to him accordingly, suggesting that he should try the effect of lifting before capping. This led him to try the effect of exposure with a moving camera, with the result that there appears to be no doubt now that such was the origin of the supposed effects. In his reply, enclosing photographs taken with a camera which was purposely moved, Mr. Webb writes: "I had made so sure that there was no shake of my camera, in spite of your frequent suggestions to the contrary, that I cannot even now understand it, placed as it was on the balcony rail, excepting in the No. 6 or five-flash exposure, when I wilfully raised or depressed the camera a little to avoid getting two of the horizontal flashes on the same plane of horizon."

I must now refer briefly to the two arguments I used (p. 413) in support of the reality of the effects. I pointed out that in Fig. 4 there was a real decrease of scale in the luminosities about the nine more distant lamps, in accordance with the increasing distance from the camera. I confess it seemed to me that the difference of scale, though real, was not as great as I should have expected, but I had no measurements of the distances of the several lamps from the camera whereby to calculate what the difference of scale should have been if the luminosities were real, and of the same size for the different lamps. A difference of scale might be produced in a moving camera if the exposed plate had a movement of rotation about the line of sight. The other argument was founded on the visibility of the discharge. It seemed to me that such a discharge as those shown for instance in Fig. 1, taken as real, might be expected to be seen directly if the eye were defended from too much glare of the lightning, and I suggested to Mr. Webb to be on the look-out if an occasion should occur. He states in his letter to NATURE (p. 343), that he actually saw such a discharge. I think it is not difficult to reconcile this with the supposition that there is no real discharge. The observer on the look-out would have his eye directed to the lamp, and when the flash came might unintentionally look in a somewhat different direction. In the rapid rotation of the eyeballs the image of the lamp would leave a trail on the retina, which might easily be mistaken for an actual luminous discharge.²

The beading of the discharge now presents no difficulty. Indeed, the first idea which naturally occurs to one on seeing it is that it might be connected with the rapid alternation of the current; but so long as the picture is supposed to represent a real discharge, it seems difficult to imagine how the alternation could possibly account for the beading.

Cambridge, March 23.

G. G. STOKES.

The Absorption of the Becquerel Rays by Solid and Gaseous Bodies.

I WISH in this note to give some observations recently made with regard to the absorption of the Becquerel rays. Though the experiments are not complete, it is hoped that the results

¹[The photographs referred to were taken by Mr. J. Williamson, of Hove, the electric lamps towards which his camera was directed being those along King's-road, Brighton. The effects were produced by giving a stand exposure of from five to ten seconds, and then moving the camera about for a few seconds with the cap still off.—Editor, NATURE.]

²Mr. Webb has suggested to me another explanation.

already obtained may be of sufficient interest to justify preliminary publication. The experimental details will be more appropriately given with the completed experiments.

Curie has shown that the rays from active barium compounds are of two kinds. One kind is easily absorbed, and is not deflectable by the magnet. The other kind is much more penetrating, and does suffer deflection in a magnetic field. It is to the latter kind exclusively that the experiments refer.

The intensity of the radiation was measured by the electrical conductivity of air exposed to it. It was again measured after partial absorption by a plate of the material under investigation.

In the following table the first column gives the coefficients of absorption λ defined by the equation

$$r = r_0 e^{-\lambda d},$$

while r_0 , r , are the initial and final intensities of the radiation, and d the distance traversed.

Material	Coefficient of absorption	Density	Coefficient of absorption Density
Platinum ...	157.6	21.5	7.34
Lead ...	62.5	11.4	5.48
Silver ...	65.7	10.6	6.20
Copper ...	49.2	8.95	5.50
Iron ...	52.2	7.76	6.74
Tin ...	51.2	7.3	7.01
Zinc ...	40.3	7.2	5.58
Mica ...	10.8	2.74	3.94
Glass ...	12.5	2.73	4.58
Aluminium ...	11.6	2.7	4.30
Celluloid ...	5.45	1.36	4.01
Ebonite ...	4.77	1.14	4.18
Card ...	3.84	1.0	3.84
Sulphur dioxide ¹	.0413	.00758	5.45

It will be seen that, although the coefficient of absorption is not accurately proportional to the density, yet the departure from this relation is not very great, if the enormous range of density be taken into account. Thus between solid platinum and the compressed sulphur dioxide used, there is a three thousand-fold difference of density. The quotients $\frac{\text{absorption}}{\text{density}}$

are respectively 7.3 and 5.45. It is interesting to compare these results with Lenard's observations on the absorption of the cathode rays (*Wied. Ann.* vol. lvi. p. 255). He found that the above relation between absorption and density held to about the same degree of approximation. The coefficients of absorption for the cathode rays are, however, some five hundred times greater than for the rays investigated in my experiments.

We may, I think, fairly consider that the approximate proportionality between absorption and density is an additional argument in favour of the view that the deflectable Becquerel rays are of the same nature as the cathode rays. To account for the enormously greater penetrating power of the latter, one must suppose either that the particles constituting them are much smaller, or that their velocity is much greater.

R. J. STRUTT.

Planets at their Greatest Brilliancy.

MR. DENNING'S able and lucid article upon the planet Mercury (*NATURE*, March 1) induces me to send a few notes. With inclined elliptical orbits it is a complicated matter to determine when an interior planet is at its greatest brilliancy. But if the orbits are assumed circular and coplanar, interesting results are easily obtained.

Theory shows that there is a certain elongation, at which the interior planet, viewed from the exterior one, has a maximum brightness. Now, for a given elongation, there are two distances, a long and a short one, between the planets. Consider only eastern elongations. It will be found that Mercury has its greatest brilliancy (for mean distances and circular orbits) when its elongation is $22^\circ 19'$, and when its distance (1.00) from the earth is the larger of the two distances possible for this elongation. The illuminated phase is 0.60. Thus Mercury is brightest *before* its maximum eastern elongation of $22^\circ 47'$.

¹ Saturated vapour at 13°C .

Venus has its greatest brilliancy at elongation $39^\circ 43'$; but its distance (0.43) from the earth must be the smaller of the two possible ones. The phase is 0.27. Thus Venus is brightest *after* its maximum elongation of $46^\circ 20'$.

But, if from Venus we view Mercury, then (as in the case of the earth and Venus) we must take the shorter distance for maximum brilliancy. The elongation is $31^\circ 36'$, distance 0.54, phase 0.40. Thus Mercury, seen from Venus, is brightest after its maximum eastern elongation of $32^\circ 21'$.

That a planet should be brightest exactly at maximum elongation involves, I find, the following relationship between the radii vectores: the radius vector of the exterior planet should be just $\sqrt{5}$ times that of the interior one. When the factor exceeds $\sqrt{5}$, the interior planet is brightest before maximum elongation. When the factor falls short of $\sqrt{5}$, the interior planet is brightest after maximum elongation. Circular orbits are assumed. For the pairs, Mercury-Venus, Venus-Earth, Earth-Mars, Jupiter-Saturn, the factor is less than $\sqrt{5}$. But for Mercury-Earth it is greater; hence Mercury is brightest before maximum elongation east, a fact clearly brought out by Mr. Denning's observations. On several occasions I have seen Mercury with the unaided eye, and, generally, after greatest eastern elongation, when the conditions are less favourable than before it.

C. T. WHITMELL.

Leeds, March 5.

P.S.—The American *Ephemeris* for 1900 shows that the maxima of brightness for Mercury occur very irregularly. One maximum occurs 6 days before greatest east elongation, another only $1\frac{1}{2}$ days after superior conjunction. Eccentricity accounts for these irregularities.

The Use of Silica in Thermometry.

I HAVE just learnt from your last number (p. 521) that Mons. A. Dufour has recently exhibited two silica thermometers in Paris, and that he proposes to study the suitability of silica for use in thermometers.

As I had the honour of exhibiting silica tubes of various sizes last June at the soirée of the Royal Society, and also then exhibited, in conjunction with Mr. Evans, our process for making such tubes, I am anxious at once to state that I have continued to study the applications of silica in conjunction with Mr. H. G. Lacell, and that we have at this moment the bulbs and stems of four delicate silica thermometers ready to be joined and filled as soon as their scales and some fittings are delivered. In February last we filled one of these ungraduated thermometers and tested it. It was shown to our colleague, Mr. J. E. Pearson, but was afterwards cut in two in order to alter the length of the degrees (20 mm.), as they were not quite as long as we then wished them to be.

I may add that the scales for these thermometers have been ordered, through the Cambridge Instrument Company, of Messrs. Zeiss, and that a special glass thermometer has been constructed for use in studying their zero points, which has now been in the hands of the Superintendent at Kew for some days.

Clifton, April 2, 1900.

W. A. SHENSTONE.

The Natural History Museum—A Correction.

IN a paper of mine on *Ilyopsyllus coriaceus*, which appeared recently in the *Natural History Transactions of Northumberland and Durham*, I referred to certain dissections—which had been described by Mr. Thomas Scott, and are now in the Natural History Museum at South Kensington—as having “deteriorated so as to be useless,” at the same time ascribing this statement to Prof. T. Jeffrey Bell, who had kindly examined the dissections at my request. The statement, so far as Prof. Bell's authority is concerned, is not quite accurate, and at his request I wish to be allowed to correct it in your columns. What Prof. Bell told me was that the dissections consist of “nothing but unrecognisable fragments,” and that “Mr. Pocock, who had charge of the Crustacea in 1893, says the tube came there in the state it is in now.”

I think I need scarcely add that my words, as quoted above, were not meant in any way to impute negligence or want of care to the officials of the Museum.

G. S. BRADY.

The Durham College of Science, Newcastle-upon-Tyne,
March 29.